

focusing on the art of Rapa Nui and Te Henua 'Enana) with accompanying high quality catalogs that bring a national and international spotlight to Pacific cultures and their rich artistic masterworks.

REFERENCE

- Kjellgren, E. and C. Ivory. 2005. *Adorning the World: Art of the Marquesas Islands*. New Haven and London: The Metropolitan Museum of Art, NY and Yale University Press.

Diffusionism Reconsidered: Linguistic and Archaeological Evidence for Prehistoric Polynesian Contact with Southern California

Terry L. Jones and Kathryn A. Klar
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Review and Discussion by Scott Nicolay

ONE OF THE MOST EAGERLY AWAITED PAPERS in anthropology is at last in print: Terry Jones and Kathryn Klar's case for transoceanic contact between Polynesians and the Chumash of southern California. It is appropriate that their paper should appear in an important venue as *American Antiquity*, as it has implications for the entire discipline of archaeology, extending beyond the two geographic areas upon which it focuses.

I will summarize the authors' ideas only briefly here, as it is not my purpose to rehash their entire thesis, but rather to discuss their paper and its implications. Basically, Jones and Klar argue that the Chumash sewn-plank canoe may be evidence for prehistoric contact with Polynesian voyagers. They suggest that Polynesians may have reached southern California, probably during the settlement of Hawai'i, and shared the secrets of the sewn-plank canoe and its construction. This is not an easy case to make, at least for legitimate anthropologists, as diffusionist arguments have long been considered the province of the "fringe" and not proper subject matter for anyone with a long-term interest in a career in the field. Jones and Klar deserve credit for their bravery in tackling the controversial issue of trans-oceanic diffusion with such diligence and professionalism and assembling such a thoroughly researched and well-constructed argument. They have given us a well-written, exhaustively researched paper, with a robust background presentation on all relevant areas, presented nonetheless in a manner both concise and clear. From now on, this paper should be held up as an example to those who seek to present such arguments without following the ground rules of the discipline. Those who cannot meet this standard do not deserve to be taken seriously. Jones and Klar definitely do.

Nota bene that this is not Heyerdahl's argument. Heyerdahl proposed the actual settlement of Polynesia from the Americas, and his case was based on a disassociated hodgepodge of "evidence" derived mostly from iconographic similarities; i.e. there are large statues on Rapa Nui with their hands folded over their stomachs; there are large statues in South America with their hands folded over their stomachs. And, of course, the back wall of Ahu Vinapu bears a superficial similarity to Inca stonework. Heyerdahl was an ultra-

diffusionist, who eventually went public with his claim that South America was originally settled from the Old World, and ultimately from northern Europe (his ubiquitous red-haired skeletons). Jones and Klar make a tightly focused case for only limited diffusion, with Polynesians as the active agents. That this happened in at least one other location is evidenced by the presence of the sweet potato in pre-contact Island Oceania, diffusion's only real smoking gun in the Pacific. Indeed, given our present knowledge of Polynesian navigation, it is likely that they visited every continent except Africa and Europe during their diaspora (and their Austronesians cousins who settled Madagascar obviously made Africa). But that is beyond the scope of Jones and Klar's paper: they argue for only a single voyage from eastern Polynesia to North America, and unlike Heyerdahl's scattershot approach, they focus their arguments almost exclusively on a single technology: the sewn-plank canoe.

Sewn-plank canoes are well documented in many locations in the Pacific, but only one other location in the Americas: the Gulf of Coronado in central Chile. Parallels exist not only between the canoes themselves, but also in the technology of their construction, suggesting actual cultural contact and not just the accidental discovery of an empty canoe by the Chumash. Most notable in this context is the perpendicular handheld adze, a distinctive component of the Polynesian toolkit. Among the Chumash, adzes were made from shell, less common than stone in Polynesia, but prevalent in some island groups. Coincidentally, Polynesian shell adzes are a topic of the paper by Ernest Winterhoff in this edition of the *Rapa Nui Journal*. This implies not the accidental appearance of an empty canoe, but a visit long enough to include lessons in the techniques and tools of its construction, just as the acquisition of the sweet potato must have involved some instruction in planting.

Appearing at the same time in the Chumash archaeological record as evidence for the sewn-plank canoe are two-piece bone fishhooks. Both the Chumash and the Polynesians used very similar shellfish hooks as well, but the evidence in that case actually suggests independent invention. These two technologies, the sewn-plank canoe and the two-piece hook, are linked in the archaeological record with a major increase in the amounts of pelagic fish remains such as swordfish and tuna, and with an increase in social complexity that made the Chumash unique among the many southern California tribes.

Equally, if not more compelling, is their linguistic data, primarily Klar's contribution. Unlike previous linguistic arguments for Polynesian-American connections, which were shallow and sloppy, Jones and Klar present a tightly constructed case that the Chumash word for the sewn-plank canoe, which they reconstruct in its earliest form as *tomolo*, actually derives from a Polynesian construction: *tumu raa'au*, a phrase they suggest probably meant "useful wood." Why not some form of *waka*, the ubiquitous Polynesian word for boat? The Chumash already had a word for boat. Here again, the proposed derivation points not so much to the artifact as to the technology, supporting the argument for actual cultural contact and an emphasis on the construction of the sewn-plank canoe. As the authors emphasize, the word itself is artifact here. Their argument gains particular weight from the fact that *tomolo* is morphologically and etymologically opaque in Chumash; that

is to say that it can neither be broken down further into meaningful parts, nor can it be connected with the Chumash languages through any reconstruction of its etymology. It stands alone and indivisible, facts which support its origin as a loan word, and one deriving from Chumash's nearest linguistic neighbor to the west: a Polynesian tongue.

Jones and Klar's arguments indirectly raise at least one question that is of particular interest to Easter Island studies. The correlation between a significant increase in Chumash population and social complexity and the advent of sewn-plank canoe technology, which they attribute to the increase in pelagic fishing that this technology made possible, is central to their argument. Nor are they alone in this; it is the opinion of a large portion of the archaeological community, as they acknowledge, and they summarize this stand carefully. In my opinion, anyone familiar with Easter Island archaeology will recognize that this is the reverse of a key component of the ecological collapse theory of Rapanui prehistory as presented by Bahn and Flenley, Diamond, and others: that the switch from sewn-plank canoe to dugout technology on Rapa Nui shows a correlation with a rapid *decrease* in population and social complexity due to the loss of pelagic fishing ability. It is true that the Chumash had a particular advantage in their sewn-plank canoe construction: abundant sources of natural tar with which to seal their canoes. In fact, asphalt plugs from canoes found in archaeological contexts are one of the earliest pieces of evidence for the *tomolo*. Nonetheless, in their discussion of Polynesian watercraft, Jones and Klar also discuss the extensive use of sewn-plank canoes in long distance voyaging in Polynesia. Could a technology that facilitates pelagic fishing in one culture herald its end in another? The pun is perhaps as unforgivable as it is inevitable, but this plank of Bahn and Flenley's thesis may not hold water.

Jones and Klar honed their argument through feedback from public presentations at several years of Society for American Archaeology meetings and drew on the advice of a wide range of experts. Instead of going on the defensive and setting themselves against the scholarly community, or attempting to circumvent the peer review process by parading their ideas in the popular media, they undertook at least two major revisions of this paper prior to its ultimate publication. Thus they have presented us with an argument that must be taken seriously. And it is. In a special symposium on April 2, 2005 at the annual SAA meeting in Salt Lake City, their ideas were received very favorably and without any of the expected antagonism. The general consensus among most of the half dozen discussants was that although Jones and Klar had not proven their case *conclusively*, they had definitely made a real and legitimate case, one that deserves serious attention and further research. Arguments for transoceanic diffusion have an extremely high bar in archaeology; Jones and Klar have passed that bar. Without question, this has to be one of the most sober and systematic cases for transoceanic diffusion ever proposed.

However one feels about the argument they advance, there is no question Jones and Klar have made a real contribution not only to both Polynesian and Chumash studies, but to the field of anthropology overall. Their paper is an exemplary piece of scholarship, their presentation impeccable,

their evidence impossible to dismiss. They have provided a model for the responsible presentation of arguments for limited diffusion, and any responsible discussion of this topic hereafter, in whatever part of the world it is set, must draw on their foundation. As Jones and Klar write: "Recognition of this apparent case of transoceanic contact suggests that diffusion and other forms of historic contingency still need to be considered in archaeological conceptualizations of North American prehistory" (Jones and Klar 2005:458). This does not mean, however, that they have opened the door to the wild-eyed fringe; instead, they have done exactly the opposite: they have taken back diffusion as a legitimate topic of discussion within the discipline of anthropology.

Island at the End of the World: The Turbulent History of Easter Island

By Steven Roger Fischer, 2005

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Review by Scott Nicolay

MANY READERS OF THIS JOURNAL will already have an entire bookshelf – or at least a large part of one – dedicated entirely to Rapa Nui. However, until now, those shelves will have lacked a straightforward and complete history of the island and its people. So much has been written about the island's prehistory that it has been easy to ignore the absence of an actual history. Only after one begins to read *Island at the End of the World* does it become obvious what has been missing all this time.

Although the outline of Rapa Nui history has long been available from a range of sources, in varying degrees of completeness and accuracy, Fischer fleshes out the full story for the first time, down to the details. Previously an overview of Rapanui history was best obtained by reading the reports of the major expeditions, along with a few other texts on special topics, such as Porteous' 1981 *The Modernization of Easter Island*. Even then, however, there were still large gaps, and many readers were left wanting to know more about such major players in Rapanui history as Alfonso Rapu and the prophetess Angata, of whom we previously had only the historical equivalent of snapshots. The complete story has never been told – not in one voice, in one piece, and with all

